working paper
department
of economics

How Large are the Social Returns to Education?
Evidence from Compulsory Schooling Laws*

Daron Acemoglu
Joshua Angrist

No. 99-30 October 1999

massachusetts
institute of
technology
50 memorial drive
cambridge, mass. 02139

WORKING PAPER
DEPARTMENT
OF ECONOMICS

How Large are the Social Returns to Education? Evidence from Compulsory Schooling Laws*

Daron Acemoglu
Joshua Angrist

No. 99-30 October 1999

MASSACHUSETTS
INSTITUTE OF
TECHNOLOGY

50 MEMORIAL DRIVE
CAMBRIDGE, MASS. 02142

How Large Are The Social Returns to Education? Evidence from Compulsory Schooling Laws*

Daron Acemoglu — MIT

Joshua Angrist — MIT

October 29, 1999

Abstract

Average schooling in US states is highly correlated with state wage levels, even after controlling for the direct effect of schooling on individual wages. We use an instrumental variables strategy to determine whether this relationship is driven by social returns to education. The instruments for average schooling are derived from information on the child labor laws and compulsory attendance laws that affected men in our Census samples, while quarter of birth is used as an instrument for individual schooling. This results in precisely estimated private returns to education of about 7 percent, and small social returns, typically less than 1 percent, that are not significantly different from zero.

Keywords: human capital externalities, returns to schooling, wage equations

JEL Classification: I20, J31, J24, D62, O15.

*We thank Chris Mazingo and Xuanhui Ng for excellent research assistance. Paul Beaudry, Bill Evans, Larry Katz, Enrico Moretti, Jim Poterba, Robert Shimer, and seminar participants at the 1999 NBER Summer Institute, the University of Maryland, and the University of Toronto provided helpful comments. Special thanks to Stefanie Schmidt for helpful advice on compulsory schooling data.

1 Introduction

Many studies find that an additional year of schooling increases individual wages by 6-10 percent. The consequences of a change in average schooling may differ from this private return, however. Changes in average education levels raise wages by less than the private returns to schooling if schooling has signaling value in addition to raising productivity, or if some other factor of production is supplied inelastically. On the other hand, the social value of education may exceed the private return because of changes in relative wages, or because of human capital externalities from a more educated labor force. Despite the potential importance of this question for economic policy, much less is known about the social returns to education than the private returns. In this paper, we develop a framework for estimating social returns and apply it to data from US states.

The principal challenge in any effort to estimate the effects of education on wages is identification. Individual education and average schooling levels are correlated with wages for a variety of reasons, so the observed association between schooling variables and wages is not necessarily causal. We use instrumental variables to estimate the effect of both individual schooling and the average schooling level in an individual's state. The instruments for average schooling are derived from compulsory attendance laws and child labor laws in states of birth. Together we refer to these as Compulsory Schooling Laws (CSLs). The problem of estimating social returns is complicated by the fact that there are two endogenous regressors affected by CSLs, individual schooling and average schooling. We solve this problem by using quarter of birth dummies as instruments for individual schooling, as in Angrist and Krueger (1991), while using CSLs as instruments for average schooling.

State CSLs generate an attractive "natural experiment" for the estimation of social returns for a number of reasons. Although these laws were determined by social forces in the states at the time of passage, the CSLs that affected an individual in childhood are not affected by future wages. We also show that CSLs affected schooling almost exclusively in middle school and high school grades. This suggests that CSLs are not correlated with omitted state-of-birth and cohort effects, since these effects would likely be related to college-going behavior as well. Finally, changing CSLs were part of the 1910-1940 "high school movement" that Goldin (1998) has argued was responsible for much of the human capital accumulation in the US in the twentieth century.

The bulk of the empirical work in the paper uses samples of white men aged 40-49 from

the 1960-80 Censuses, although some estimates are computed using an extended sample that includes 1950 and 1990 data. Blacks are excluded from the estimation because cohorts of blacks in these data sets experienced marked changes in school quality (see, e.g., Card and Krueger, 1992a; Margo, 1990; Welch, 1973). We focus on the 1960-80 Censuses because they include information on quarter of birth and because the census schooling variable changes in 1990. The fact that men in their 40s are on a relatively flat part of the age-earnings profile also facilitates the use of quarter of birth dummies as instruments.

OLS estimates using data from the 1960-80 Censuses show a large positive relationship between average schooling and individual wages. A one-year increase in average schooling is associated with a roughly 7 percent increase in average wages, over and above the roughly equal private returns. In contrast with the OLS estimates, IV estimates of social returns in 1960-80 are small, typically less than one percent, not significantly different from zero, and significantly lower than the corresponding within-state OLS estimates. Adding data from the 1990 Census results in somewhat larger estimates of social returns, but this finding seems to be generated at least in part by problems with the schooling variable in the 1990 Census.

In addition to raising overall wage levels, as in our model of social returns, exogenous changes in aggregate schooling levels may change the private returns to schooling. Changes in average schooling caused by compulsory schooling laws provide an attractive source of variation to estimate the effect of supply shocks on the private returns to schooling. We explore this issue by simultaneously estimating social returns and changes in private returns to schooling. The results of this analysis also provide no evidence for significant social returns. They suggest, however, that the returns to education may actually be somewhat higher in states with greater average schooling.

Previous studies of the social returns to education include Rauch (1993), Mare (1995), Peri (1998), and Moretti (1999), who estimate the effect of average schooling in US cities on individual wages, and Topel (1999), who estimates social returns using cross-country data on education and labor productivity. Rauch, Mare, and Peri treat both individual and average schooling variables as exogenous. In contrast, Moretti (1999) instruments for average schooling with changes in city age structure, tuition costs and the presence of a land-grant college, though he treats individual schooling as exogenous. We show that instrumenting average schooling but not individual schooling may also be misleading

if instrumental variables (IV) estimates of private returns differ from OLS estimates. Another difference between our paper and Moretti's is that compulsory schooling laws mainly affected pupils in middle school or high school, while most of the variation in Moretti's sample comes through changes in college attendance and graduation.

The next section lays out a simple economic model that shows how human capital externalities can arise. This model is used to develop an estimation framework and to highlight the econometric issues raised by human capital externalities. Section 3 discusses the data and reports OLS estimates from regressions on individual and average schooling. Section 4 describes the CSL instruments, Section 5 reports the IV estimates, and Section 6 estimates the impact of greater supply of education on private returns to schooling.

2 Social returns: theory and measurement

2.1 Theoretical framework

A model similar to Acemoglu (1996) is used to highlight the economic forces that may generate social returns to education, and to derive an estimating equation. Consider an economy lasting two periods, with production only in the second period, and a continuum of workers normalized to 1. An individual's human capital is given by

$$h_i = \exp(\eta_i \cdot s_i),$$

where s_i is worker i's schooling. Workers have unobserved ability $\eta_i = \theta_i \eta(s_i)$, which depends on an individual characteristic, θ_i, and also potentially on schooling. This dependence captures potential decreasing returns to individual schooling, as in Lang (1993).

A worker's consumption, C_i, is equal to his labor income. Schooling is chosen by workers so as to maximize

$$\ln C_i - \frac{1}{2}\psi_i \cdot s_i^2. \tag{1}$$

The parameter ψ_i is the cost of education for individual i and can be interpreted as a personal discount rate, along the lines of Card (1995a).

There is also a continuum of risk-neutral firms. In period 1, firms make an irreversible investment decision, k, at cost rk. Workers and firms come together in the second period. The labor market is not competitive; instead, firms and workers are matched randomly, and each firm meets a worker. The only decision workers and firms make after matching is

whether to produce together or not to produce at all (since there are no further periods). If firm j and worker i produce together, their output is

$$k_j^\alpha \cdot h_i^\nu \tag{2}$$

where $\alpha < 1$, $\nu \leq 1-\alpha$, and the worker receives a share β of this output as a result of bargaining.

An equilibrium in this economy is a set of schooling choices for workers and a set of physical capital investments for firms. Firm j maximizes the following expected profit function:

$$(1-\beta) \cdot k_j^\alpha \cdot E[h_i^\nu] - r \cdot k_j, \tag{3}$$

with respect to k_j. Since firms do not know which worker they will be matched with, their expected profit is an average of profits from different skill levels. The function (3) is strictly concave, so all firms choose the same level of capital investment, $k_j = k$, given by

$$k = \left(\frac{(1-\beta)\cdot\alpha\cdot H}{r}\right)^{1/(1-\alpha)} \tag{4}$$

where

$$H \equiv E[h_i^\nu] = E[\exp(\nu \cdot \eta_i \cdot s_i)]$$

is the measure of aggregate human capital. Substituting (4) into (2), and recalling that wages are equal to a fraction β of output, the wage income of individual i is given by $W_i = \beta\left(\frac{(1-\beta)\alpha H}{r}\right)^{\alpha/(1-\alpha)}(\exp(\eta_i s_i))^\nu$. Taking logs, this is:

$$\begin{aligned}\ln W_i &= c + \frac{\alpha}{1-\alpha}\ln H + \nu\eta_i s_i, \\ &= c + \frac{\alpha}{1-\alpha}\ln H + \nu \ln h_i,\end{aligned} \tag{5}$$

where c is a constant and $\frac{\alpha}{1-\alpha}$ and ν are positive coefficients.[1]

Human capital externalities arise here because firms choose their physical capital in anticipation of the average human capital of the workers they will employ in the future.

[1] As in Acemoglu (1996), human capital externalitites are additive in logs, so the marginal product of a more skilled worker increases when the average workforce skill level increases. Acemoglu (1998, 1999) discuss models in which log wage *differences* between skilled and unskilled workers increase with average skill levels.

Since physical and human capital are complements in this setup, a more educated labor force leads to greater investment in physical capital and to higher wages. In the absence of the need for search and matching, firms would immediately hire workers with skills appropriate to their investments, and there would be no human capital externalities.[2]

The model is completed by individual schooling decisions, which are determined by maximizing (1), taking (5) as given. This yields equilibrium schooling levels satisfying

$$\begin{aligned} \nu\theta_i\left[\eta(s_i) + s_i\eta'(s_i)\right] &= \psi_i s_i, \text{ or} \\ \eta'(s_i)\left(\varepsilon_\eta^{-1} + 1\right) &= \frac{\psi_i}{\nu\theta_i}, \end{aligned} \tag{6}$$

where ε_η is the elasticity of the function η. The population average return to optimally chosen schooling levels is $E[\nu\theta_i(\eta_i(s_i) + s_i\eta_i'(s_i))]$. But the average return for particular subpopulations interacts with discount rates in a manner noted by Lang (1993) and Card (1995a). For example, if $\eta'(s_i) < 0$, those with high ψ_i will get less schooling, and a marginal year of schooling will be worth more to such people than the population average return.

Equation (5) provides the theoretical basis for our empirical work. Since H is unobserved, however, we use the following approximation to write $\ln H$ as a function of average schooling:

$$\ln H = \ln E[\exp(\nu\eta_i s_i)] \approx c_0 + c_1 E(\eta_i s_i) \approx c_2 + c_3 E(s_i).$$

The first step approximates the mean of the log with the log of the mean. The second step takes $E(\eta_i)$ and the covariance between η_i and s_i to be constant, unaffected by changes in average education. Estimation can therefore be based on the following equation for individual i residing in state j:

$$\ln W_{ij} \approx \gamma_0 + \gamma_1\overline{S}_j + \gamma_2\eta_i s_i \tag{7}$$

where $\overline{S}_j = E(s_i)$ is average schooling in state j.

The scenario outlined here is not the only mechanism for positive social returns. One possibility, noted by Marshall (1964), Jacobs (1970), and Lucas (1988), is that workers learn from each other in local labor markets. There can also be (local) human capital

[2] In a frictionless world, firms maximize profits conditional on realized worker-firm matches instead of conditional on the expected match. In this case, firm j matched to worker i chooses capital $k_j = \left(\frac{\alpha h_i^\nu}{r}\right)^{1/(1-\alpha)}$ and worker i's wage is $\ln W_i = c' + \frac{\nu\alpha}{1-\alpha}\ln h_i$.

externalities if more educated workers produce higher quality intermediate goods, and monopolistically competitive upstream and downstream producers locate in the same area. Our empirical strategy does not attempt to distinguish between these mechanisms and the one outlined in this section, since they have similar implications.

2.2 Econometric framework

This section discusses instrumental-variables strategies to estimate equation (7), the causal relationship of interest.[3] In practice, of course, there are many factors beside schooling that determine wages. An error term is therefore added to the estimating equation. Also, we adopt notation that reflects the fact that different individuals are observed in different years in our data. The resulting equation is

$$Y_{ijt} = X_i'\mu + \delta_j + \delta_t + \gamma_1 \overline{S}_{jt} + \gamma_{2i} s_i + u_{jt} + \varepsilon_i, \tag{8}$$

where Y_{ijt} is the log weekly wage, u_{jt} is a state-year error component, and ε_i is an individual error term. The vector X_i includes state-of-birth and year-of-birth effects, and δ_j and δ_t are state-of-residence and Census year effects. The random coefficient on individual schooling is $\gamma_{2i} \equiv \gamma_2 \eta_i$, while the coefficient on average schooling, γ_1, is taken to be fixed.

The most important identification problem raised by equation (8) is omitted variables bias from correlation between average schooling and other state-year effects embodied in the error component u_{jt}. For example, economic growth may increase wages in a state, while also increasing the demand for schooling. To solve this problem, the CSLs that different cohorts were exposed to at age 14 are used to construct instruments for $\overline{S}_{jt}$. CSL instruments are correlated with individual schooling because different cohorts in each state were exposed to different CSLs. Since people tend to live in their states of birth, this generates correlation with average schooling in states of residence

While omitted state-year effects are the primary motivation for this IV strategy, the fact that one regressor, $\overline{S}_{jt}$, is the average of another regressor, s_i, also complicates the interpretation of OLS estimates. To see this, consider an "atheoretical" regression of Y_{ij} on both s_i and $\overline{S}_j$, which for purposes of illustration is assumed to have constant coefficients and a cross-section dimension only:

$$Y_{ij} = \mu^* + \pi_0 s_i + \pi_1 \overline{S}_j + \xi_i; \text{ where } E[\xi_i s_i] = E[\xi_i \overline{S}_j] \equiv 0. \tag{9}$$

[3] Brock and Durlauf (1999) survey non-instrumental variables approaches to estimating models with social effects.

Now, let ρ_0 denote the coefficient from a bivariate regression of Y_{ij} on s_i only and let ρ_1 denote the coefficient from a bivariate regression of Y_{ij} on $\overline{S}_j$ only. Note that ρ_1 is the two-stage least squares (2SLS) estimate of the coefficient on s_i in a bivariate regression of Y_{ij} on s_i using a full set of state dummies as instruments. The appendix shows that

$$\begin{aligned} \pi_0 &= \rho_1 + \phi(\rho_0 - \rho_1) \\ \pi_1 &= \phi(\rho_1 - \rho_0) \end{aligned} \tag{10}$$

where $\phi = \frac{1}{1-R^2} > 1$, and R^2 is the R-squared from a regression of s_i on state dummies. Thus, if *for any reason* OLS estimates of the bivariate regression differ from 2SLS estimates using state-dummy instruments, the coefficient on average schooling in (9) will be nonzero. For example, if grouping corrects for attenuation bias due to measurement error in s_i, we would have $\rho_1 > \rho_0$ and the appearance of positive social returns even when $\gamma_1 = 0$ in (8). In contrast, if grouping eliminates correlation between s_i and unobserved earnings potential, we would have $\rho_1 < \rho_0$, and the appearance of negative social returns.[4] This problem becomes more severe when returns to education vary across individuals as in our random coefficients specification, (8). Nevertheless, an instrumental variables strategy that treats both s_i and $\overline{S}_j$ as endogenous can generate consistent estimates of social returns. The key to the success of this approach is finding the right instrument for individual schooling.

To develop this point more formally, consider a simplified version of the random coefficient model (8), again with no covariates and no time dimension. Assume also that a single binary instrument is available to estimate γ_1, say z_i, a dummy for having been born in a state with restrictive CSLs. Finally, suppose we adjust for the effects of s_i by subtracting $\gamma_2^* s_i$, where γ_2^* is some average of γ_{2i}. In other words, subtract $\gamma_2^* s_i$ from both sides of (8) to obtain

$$\begin{aligned} Y_{ij} - \gamma_2^* s_i &\equiv \widetilde{Y}_{ij} \\ &= \mu + \gamma_1 \overline{S}_j + [u_j + \varepsilon_i + (\gamma_{2i} - \gamma_2^*) s_i]. \end{aligned} \tag{11}$$

What value of γ_2^* allows us to use z_i as an instrument for $\overline{S}_j$ in (11) to obtain a consistent estimate of γ_1? The instrumental variables estimand in this case, γ_1^{IV}, is given by the

[4] The coefficient on average schooling in an equation with individual schooling can be interpreted as the Hausman (1978) test statistic for the equality of OLS estimates and 2SLS estimates of private returns to schooling using state dummies as instruments. Borjas (1992) discusses a similar bias in the estimation of ethnic-background effects.

Wald formula:

$$\begin{aligned}
\gamma_1^{IV} &= \frac{E[\widetilde{Y}_{ij}|z_i=1]-E[\widetilde{Y}_{ij}|z_i=0]}{E[\overline{S}_j|z_i=1]-E[\overline{S}_j|z_i=0]} \\
&= \gamma_1+\left[\frac{E[\gamma_{2i}s_i|z_i=1]-E[\gamma_{2i}s_i|z_i=0]}{E[s_i|z_i=1]-E[s_i|z_i=0]}-\gamma_2^*\right]\cdot\left[\frac{E[s_i|z_i=1]-E[s_i|z_i=0]}{E[\overline{S}_j|z_i=1]-E[\overline{S}_j|z_i=0]}\right].
\end{aligned}$$

This shows that γ_1^{IV} estimates social returns to education consistently (i.e., equals γ_1) if the adjustment for individual schooling uses the coefficient:

$$\begin{aligned}
\gamma_2^* &= \frac{E[\gamma_{2i}s_i|z_i=1]-E[\gamma_{2i}s_i|z_i=0]}{E[s_i|z_i=1]-E[s_i|z_i=0]} \\
&= \frac{E[(Y_{ij}-\gamma_1\overline{S}_j)|z_i=1]-E[(Y_{ij}-\gamma_1\overline{S}_j)|z_i=0]}{E[s_i|z_i=1]-E[s_i|z_i=0]}.
\end{aligned} \tag{12}$$

In other words, the adjustment for effects of s_i should use the (population) IV estimate of *private returns* generated by z_i, once we subtract the effect of human capital externalities.

Of course, we cannot use z_i to estimate both private and social returns, even though (12) appears to require this. But instruments based on quarter of birth can be used to estimate γ_2^*. Let q_i denote a single instrument derived from quarter of birth, say a dummy for first quarter births. Since q_i is orthogonal to $\overline{S}_j$, we have

$$\gamma_q^* = \frac{E[Y_{ij}|q_i=1]-E[Y_{ij}|q_i=0]}{E[s_i|z_i=1]-E[s_i|z_i=0]} = \frac{E[\gamma_{2i}s_i|q_i=1]-E[\gamma_{2i}s_i|q_i=0]}{E[s_i|q_i=1]-E[s_i|q_i=0]}.$$

If $\gamma_q^*=\gamma_2^*$, the quarter-of-birth instrument provides an appropriate adjustment for private returns in (11).[5]

To see why γ_q^* should be close to γ_2^*, let $w_i(s_i)\equiv\gamma_{2i}s_i$, and note that $w_i'(s_i)$ is the causal effect of schooling on i's (log) wages with $\overline{S}_j$ fixed (see equation (8)). Also, let s_{1i} denote the schooling i would get if $z_i=1$, and let s_{0i} denote the schooling i would get if $z_i=0$.[6] Angrist, Graddy, and Imbens (1995) show that

$$\gamma_2^* = \frac{\int E[w_i'(\sigma)|\ s_{1i}\geq\sigma>s_{0i}]P[s_{1i}\geq\sigma>s_{0i}]d\sigma}{\int P[s_{1i}\geq\sigma>s_{0i}]d\sigma}, \tag{13}$$

[5] In practice, we have more than one CSL instrument, so it may be possible to use CSLs to instrument s_i and $\overline{S}_{jt}$ simultaneously. Note, however, that because of the group structure of $\overline{S}_{jt}$ and the CSL instruments, the projection of s_i on the CSL instruments is almost identical to the projection of $\overline{S}_{jt}$ on the CSL instruments. This is not a problem with quarter of birth instruments since they are independent of $\overline{S}_{jt}$.

[6] These potential schooling choices can be described in terms of the theoretical framework. Suppose, for example, that $\eta(s_i)=\overline{\eta}$ and the CSL instrument changes discount rates from ψ_{0i} or ψ_{1i} as in Card (1995a). Using (6), individual schooling choices would be $s_{0i}=\frac{\nu\theta_i\overline{\eta}}{\psi_{0i}}$ and $s_{1i}=\frac{\nu\theta_i\overline{\eta}}{\psi_{1i}}$.

which is an average derivative with weighting function $P[s_{1i} \geq \sigma > s_{0i}] = P[s_i \leq \sigma | z_i = 0] - P[s_i \leq \sigma | z_i = 1]$. In other words, IV estimation using z_i produces an average of the derivative $w_i'(\sigma)$, with weight given to each value σ in proportion to the instrument-induced change in the cumulative distribution function (CDF) of schooling at that point. Similarly, γ_q^* is a CDF-weighted average with s_{1i} and s_{0i} defined to correspond to the values of q_i.

CSL instruments and quarter of birth instruments both estimate individual returns for people whose schooling is affected by compulsory schooling laws—i.e., individuals who would have otherwise dropped out of school. So the weighting functions $P[s_i \leq \sigma | z_i = 0] - P[s_i \leq \sigma | z_i = 1]$ and $P[s_i \leq \sigma | q_i = 0] - P[s_i \leq \sigma | q_i = 1]$ should be similar. In fact, we show below that, like quarter of birth instruments, CSLs changed the distribution of schooling primarily in the 8-12 range. This suggests that γ_q^* and γ_2^* capture similar features of the causal relationship between individual schooling and earnings.

3 Data and OLS estimates

3.1 Data sources

Most of the analysis uses an extract of US-born white males aged 40-49 from the 1960-80 Census microdata samples. These samples were chosen because they include data on quarter of birth, and are limited to groups on the flattest part of the age-earnings profiles.[7] This reduces bias from age or experience effects when using quarter-of-birth dummies as instruments. We also look at samples that include data from the 1950 and 1990 Censuses. Because these censuses do not include quarter of birth, estimates using the extended sample must treat individual schooling as exogenous. A second problem with the 1990 data is that the schooling variable is categorical.

The schooling variable for individuals in 1950-80 data is highest grade completed, capped at 17 years to impose a uniform topcode across censuses. Average schooling in a state and year is measured as the average of the capped highest grade completed for the full sample of workers aged 16-64 (i.e., not limited to white men). The averages

[7]Data are from the following IPUMS files (documented in Ruggles and Sobek, 1997): the 1 percent sample for 1960, Form 1 and Form 2 State samples for 1970 (giving a 2 percent sample), and the 5 percent PUMS-A sample for 1980. The 1950 sample includes all Sample Line individuals in the relevant age/sex/race group, and the 1990 data are from the IPUMS self-weighting 1 percent file. Stacked regressions are weighted so that each year gets equal weight. For additional information, see the data appendix.

are weighted by individuals' weeks worked the previous year. For 1990 data, we assigned average years of schooling to categorical values using the imputation for white men in Park (1994). Average schooling in 1990 is the average capped value of this imputed years of schooling variable.[8]

The relevant "labor market" for the estimation of equation (8) is taken to be a state. Previous work on social returns in the US has used cities, while macroeconomic studies of education and growth have used countries (e.g., Mankiw, Romer, and Weil, 1992; Barro and Sala-i-Martin, 1995; Bils and Klenow, 1998; Topel, 1999; or Krueger and Lindahl, 1999). We use states because all three PUMS samples record state of residence while the 1960 and part of the 1970 PUMS fail to identify cities or metropolitan areas. Since the instruments used here are derived from individuals' states of birth and not their cities of birth, little is lost from this aggregation.

Table 1 gives descriptive statistics for the extract. The average age is constant across censuses, while average schooling increases by slightly less than a year between 1950-60, and by slightly more than a year between 1960-70, 1970-80 and 1980-90. The mean of state average schooling, shown in the row below individual schooling, refers to the entire working age population. The standard deviation of average schooling indicates the extent of variation in this average across states. The next two rows record the lowest and highest average schooling. For example, in 1980 the lowest average education was 11.8 years, in Kentucky, while Washington, DC had the highest average education at 13.1. The last eight rows of Table 1 report the fraction in each census affected by child labor and compulsory attendance laws as we have coded them. We discuss these variables in detail in Section 4, below.

3.2 OLS estimates

OLS estimates of private returns are similar to those reported elsewhere, and do not change much with controls for average schooling. For example, the estimates show a marked increase in schooling coefficients between 1980 and 1990. This can be seen in Table 2, which reports OLS estimates of models with and without $\overline{S}_{jt}$, using pooled samples, and separately by census year. The pooled regressions include state of residence effects, year effects, year of birth effects, and state of birth effects. Regressions using

[8]Only 1 percent samples are used for the calculation of averages. Alternative weighting schemes for measures of average schooling (e.g., unweighted) generated similar results.

the individual censuses omit state of residence effects. All standard errors reported in the paper are corrected for state-year clustering using the formula in Moulton (1986). Corrected standard errors are as much as two times larger than uncorrected standard errors because of the group structure of some of the instruments and regressors.

OLS estimates of social returns for 1960-80 imply that a one-year increase in state average schooling is associated with a .073 increase in the wages of all workers in that state. Using data from 1950-80 generates an estimate of .061, whereas the 1950-90 sample leads to an estimated social return of .072. These are similar to Moretti's (1999) estimates of social returns using within-city variation, which range from .08 to .13. The estimates in Table 2 using single censuses are from models without state effects. The resulting coefficients on average schooling are considerably larger, suggesting that at least some of the relationship between average schooling and wages is due to omitted state characteristics.[9]

4 Compulsory schooling laws and schooling

4.1 Construction of CSL variables

The CSL instruments were coded from information on five types of restrictions related to school attendance and work permits that were in force at the time census respondents were aged 14. These restrictions specify the maximum age for school enrollment (*enroll_age*); the minimum dropout age (*drop_age*); the minimum schooling required before dropping out (*req_sch*); the minimum age for a work permit (*work_age*); and the minimum schooling required for a work permit (*work_sch*). Information was collected for every 3-6 years from 1914-65, and missing years were interpolated by extending older data. For example, data for cohorts aged 14 in 1924-28 come from a source for 1924. Sources for the CSLs are documented in the data appendix.

The five CSLs vary considerably over time and across states. This can be seen in Table 3, which reports the mean and standard deviation for each CSL component in the years for which we have CSL data. Statistics in the table are averages using micro data; that is, they weight state requirements using the sample distribution of states for each cohort. The data show that compulsory attendance requirements have generally been growing more restrictive, with the maximum enrollment age falling and the minimum

[9] Rauch (1993) reports cross-section estimates around .05 using data from the 1980 Census. These estimates are not directly comparable to ours because Rauch's model includes occupation dummies and average experience.

dropout age rising. The minimum age for work has also increased. The cross-section variability in age requirements for dropout and work permits has fallen over time.

Margo and Finnegan (1996) show that in the 1900s, child labor laws were at least as important as attendance restrictions for educational attainment, and the evidence presented in Schmidt (1996) suggests the same for 1920-1935.[10] We therefore combine the five CSL components into two variables, one summarizing compulsory attendance laws and one summarizing child labor laws. Compulsory attendance laws are summarized as the minimum years in school required before leaving school, taking account of age requirements. This is the larger of schooling required before dropping out and the difference between the minimum dropout age and the maximum enrollment age:

$$CA = \max\{req\_sch;\ drop\_age - enroll\_age\}$$

Similarly, child labor laws are summarized as the minimum years in school required before being allowed to work. This is the larger of schooling required before receiving a work permit and the difference between the minimum work age and the maximum enrollment age:

$$CL = \max\{work\_sch;\ work\_age - enroll\_age\}$$

These summary variables combine the CSLs into two measures that are highly related to educational attainment both conceptually and empirically.

Over 95 percent of men aged 40-49 in both the 1960-80 and 1950-90 censuses have CL in the 6-9 range, while CA is concentrated in the 8-12 range, with almost no one in the "11" category. The distribution of CL and CA can therefore be captured using four dummies for each variable. For CL, the dummies are:

$$\begin{aligned} CL6 \text{ for } CL &\leq 6, \\ CL7 \text{ for } CL &= 7, \\ CL8 \text{ for } CL &= 8, \\ CL9 \text{ for } CL &\geq 9. \end{aligned}$$

[10] Edwards (1978), Ehrenberg and Marcus (1982), Lang and Kropp (1986), and Angrist and Krueger (1991) also present evidence that compulsory schooling laws affect schooling.

Similarly, for CA, the dummies are:

$$\begin{aligned} CA8 \text{ for } CA &\leq 8, \\ CA9 \text{ for } CA &= 9, \\ CA10 \text{ for } CA &= 10, \\ CA11 \text{ for } CA &\geq 11. \end{aligned}$$

Proportions in each group are reported with the descriptive statistics in Table 1. In the empirical work, the omitted categories are the least restrictive groups for CL and CA.

4.2 CSL effects on individual schooling

There is a large and statistically significant relationship between individual schooling and both sets of CSL dummies. This is shown in Table 4a, which reports results from regressions of individual schooling on $CL7 - CL9$ and $CA9 - CA11$, along with year effects, year of birth effects, and state of birth effects. For example, the entry in column 1 shows that in the 1960-80 sample, men born in states with a child labor law that required 9 years in school before allowing work ended up with .26 more years of school completed than those born in states that required 6 or fewer years. The results are similar in models that do not include state-of-residence effects.

The right half of Table 4a shows that adding 1950 Census data to the sample leads to CSL effects similar or slightly smaller than those estimated in the 1960-80 data alone. Incorporating both 1950 and 1990 data leads to larger effects. Also, the relationship between CSLs and schooling is larger and more precisely estimated in samples that pool three or more censuses than in a sample using 1980 data only. For example, column 4 shows that with 1980 data alone, the effect of $CL9$, though still statistically significant, falls to .17.

Overall, the estimates reflect a pattern consistent with the notion that more restrictive laws caused higher educational attainment. This can be seen in Figures 1 and 2, which plot differences in the probability that educational attainment is at or exceeds the grade level on the X-axis (i.e., one minus the CDF). The differences are between men exposed to different CSLs in the 1960-80 sample, with men exposed to the least restrictive CSLs as the reference group.

Figure 1 shows that men who were exposed to more restrictive child labor laws were 1-6 percentage points more likely to complete grades 8-12. These differences decline at

lower grades, and drop off sharply after grade 12. Figure 2 shows a similar pattern for compulsory attendance laws. If CSLs cause schooling changes, and not vice versa, we would in fact expect the impact of these laws to primarily shift the distribution of schooling in middle- and high-school grades. On the other hand, if the laws pick up omitted factors related to macroeconomic conditions, tastes for schooling, or family background, we might have found that more restrictive CSLs increase the proportion attending college as well as the proportion completing high school.[11]

Table 4b quantifies the CDF differences plotted in the figures for 1960-80 and shows analogous results for the 1950-80 sample. The table reports CSL coefficients in regressions of dummy variables for whether an individual has completed the level of schooling indicated in the column heading. All of the positive differences for grades 8-12 are statistically significant. The negative differences at schooling levels above 12 are smaller and less likely to be significant. The estimates in the table also suggest that child labor laws shifted the distribution of schooling at younger grades more than compulsory attendance laws. This too is consistent with a causal interpretation of the relationship between CSLs and schooling since child labor laws refer to lower schooling levels than compulsory attendance laws. Interestingly, we replicate Margo and Finnegan's (1996) finding for the 1900s that child labor laws have been more important for educational attainment than compulsory attendance laws.

For the most part, the CDF differences in the figures and in Table 4b are ordered by increasing severity, as would be expected if these differences reflect increasingly restrictive laws. For example, using 1960-80 data, the difference at grade 9 for men with $CL9 = 1$ exceeds the difference for men with $CL8 = 1$. This in turn exceeds the difference for men with $CL7 = 1$. Adding 1950 data leaves this pattern unchanged.

A final noteworthy feature of the figures is their similarity to differences in the CDF of schooling induced by quarter of birth (as reported in Angrist and Imbens, 1995). Like CSLs, quarter of birth changes the distribution of schooling primarily in the 8-12 grade range. This supports our claim that CSL instruments and quarter of birth instruments are likely to generate similar estimates of the private return to schooling, since, as noted in Section 2.2, IV estimates implicitly weight individual causal effects using CDF differences.

[11] Up to 12th grade, the CSLs increase schooling above required levels. For example, $CL9$ makes high-school graduation more likely. This may reflect "lumpiness" of schooling decisions, peer effects, or the fact that our coding is imperfect. Lang and Kropp (1986) note that educational sorting might also lead people not affected directly by CSLs to change their schooling when CSLs change.

4.3 Private returns to education

Table 4a shows that CSLs are an important determinant of individual schooling, so they can be used as instruments for individual schooling in wage equations. On the other hand, if there are social returns to schooling, IV estimates of private returns using CSL instruments will be biased because the instruments will pick up the effect of state average schooling on earnings.[12] In fact, one simple test for social returns is to compare estimates using quarter of birth instruments, which are uncorrelated with average education, to estimates using CSL instruments.

Table 5 reports 2SLS estimates of the private returns to schooling using three different sets of instruments. Using 30 quarter of birth/year of birth dummies, the private return to schooling is estimated at .073 (with a standard error of .012). This is less than the Angrist and Krueger (1991) estimate from a similar specification using 1980 data only. Columns 2 and 3 show that the discrepancy is explained by the fact that 1960 and 1970 data generate smaller quarter-of-birth estimates than the 1980 sample.[13]

Estimates of private returns using CSL instruments in the 1960-80 sample exceed those using quarter of birth instruments, though the differences are not large or statistically significant. The 2SLS estimate of private returns using $CL6 - CL8$ as instruments, reported in column 4, is .076 (s.e.=.034). Using $CA8 - CA10$ as instruments generates an estimate of .092 (s.e.=.044), shown in column 7. Models estimated using CSL instruments without state of residence effects produce similar but less precise estimates. This loss of precision reflects the impact of state-year clustering on the corrected standard errors.

Although we present a more detailed analysis below, the fact that quarter of birth and CSL instruments generate similar schooling coefficients in the 1960-80 data already suggests that social returns are modest in this period. As noted above, significant social returns would likely lead to estimates of private returns that are biased upwards when using CSL instruments, since CSLs are correlated with average schooling. Quarter of birth instruments, on the other hand, are not subject to this bias.

Adding 1950 data to the sample leads to somewhat larger estimates with CL instru-

[12]Similarly, positive social returns may also bias IV estimates of private returns using aggregate distance instruments, as in Card (1995b).

[13]Bound, et al (1995) note that with many instruments, 2SLS estimates may be biased towards OLS estimates, and argue that this is a problem for some of the specifications reported by Angrist and Krueger (1991). However, re-analyses of these data by, among others, Chamberlain and Imbens (1996), Stock and Staiger (1997), and Angrist and Krueger (1995), suggest that using 3 quarter of birth dummies interacted with 10 year of birth dummies as instruments produces approximately unbiased estimates.

ments. Adding 1990 data as well leads to even larger estimates using CL instruments, and to a substantial increase in precision with both sets of instruments. On the other hand, the estimates using CA instruments are remarkably insensitive to the inclusion of 1950 and 1990 data.

Finally, it is noteworthy that the IV estimates using quarter of birth are very close to the OLS estimates for the same period; compare, for example, the estimates of .073 in column 1 of Table 5 and column 1 of Table 2. Thus, estimates of social returns that treat individual schooling as exogenous and endogenous should give similar results, at least for the 1960-80 sample.

5 Social returns to education

5.1 Results for 1960-80

The bottom panel of Table 6 shows the relationship between CSL dummies and *average* schooling in 1960-80 data. The first-stage equations include year, year of birth, state of birth, and state of residence dummies. CSL effects are identified in these models because cohorts born in different years in the same state were exposed to different laws. The effect of CSL dummies on average schooling is similar to, though typically somewhat smaller than, the corresponding effect on individual schooling. A moderately weaker relationship is not surprising since the average schooling variables refer to a broader group than our sample of white men in their 40s.

The instrumental variables estimates reported in the top half of the table are from models that treat both s_i and $\overline{S}_{jt}$ as endogenous. Using quarter of birth and child labor laws as instruments generates a private return of .074 (s.e.=.012) and a social return of .003 (s.e.=.040). This is considerably smaller, though less precise, than the corresponding OLS estimate of social returns. The 90 percent confidence interval for social returns, [-.065, .066], excludes the OLS estimate of .073 (see Table 2). Using compulsory attendance laws as instruments generates a somewhat higher social return, not significantly different from OLS estimates, but still considerably lower at .017 (s.e.=.043). Note, however, that the first stage for average schooling using $CA9-CA11$ is not as sharply patterned as the first stage using $CL7-CL9$.

Using both sets of CSL dummies as instruments generates a more precisely estimated social return of .004 (s.e.=.035). The 90 percent confidence interval for this estimate

is [-.053, .061], which now comfortably excludes the OLS estimate. Finally, column 4 reports results using both CL and CA dummies, and a full set of interactions between them, as instruments. This is useful because child labor and compulsory attendance laws may work together to encourage students to stay in school longer. The results in this case are somewhat more precise than estimates that do not use the interaction terms as instruments, showing social returns of 0.005 with standard error of .033.

Earlier we argued that it is important to use the "right" private return to adjust for individual schooling when estimating social returns. On the other hand, the IV estimates of private returns in columns 1-4 of Table 6 are remarkably close to the OLS estimates of private returns reported in Table 2. This suggests that estimates of social returns from models that treat individual schooling as exogenous may not be biased. Columns 5-8 in Table 6 report estimates from models that treat individual schooling as exogenous and drop the quarter of birth instruments. The estimates of social returns in columns 5-8 are indeed similar to those in columns 1-4, though slightly more precise. For example, the estimated social return using CL dummies is .002 (s.e.=.038). With both sets of CSL dummies, the estimated social return is .005 (s.e.=.032), while adding interactions between the two sets of dummies generates slightly negative social returns with a standard error of .03. Overall, the results in Table 6 offer no evidence for substantial social returns to education.

5.2 Additional estimates

Regardless of changes in sample and model specification, IV estimates using child labor laws fail to generate evidence of significant social returns,. This is documented in Table 7, which reports first- and second-stage estimates in subsamples, and results from models that allow private returns to vary by census year and state. An analysis in subsamples is worth checking since men aged 40-49 in the 1970 Census served in World War II, while those in the 1960 Census were in school during the Great Depression. Allowing for different private returns is useful since there is region and time variation in the private returns to schooling.

Estimates of social returns using child labor laws as instruments ($CL7-CL9$) change little when the sample is limited to 1960 and 1980, or to 1970 and 1980. Moreover, the first-stage estimates in these samples still show more restrictive CSLs leading to higher average schooling. These results can be seen in columns 2 and 3 of Table 7. Models

where private returns vary by year and state were estimated treating individual schooling as exogenous. Allowing private returns to vary by year generates an estimated social return of .007 (s.e.=.036), shown in column 4, while allowing private returns to vary by year and state generates an estimate of -.024 (s.e.=.039), reported in column 5.

If IV estimates of private returns were actually larger than OLS estimates, as suggested by results in some of the studies surveyed by Card (1999), then the estimates of social returns computed here would be biased upward (see section 2.2). To illustrate the implications of a higher IV estimate of private returns, we estimated social returns imposing a private return of .08 or .09 (i.e., using $Y_{ijt} - .08s_i$ or $Y_{ijt} - .09s_i$ as the dependent variable). The estimated social returns in this case, reported in columns 6 and 7, are even smaller than the estimates in columns 1-5. With private returns of 9 percent, for example, the social return is estimated to be -.018 (s.e.=.039).

Results from variations on the basic specification using compulsory attendance instruments ($CA9 - CA11$) are reported in panel B of Table 7. Estimates using 1960-80 and 1970-80 data only are notably larger than the estimates using CL instruments reported in panel A. But controlling for private returns that vary by year leads to considerably smaller estimates, and controlling for private returns that vary by state and year leads to a negative social return. Overall, therefore, the results in Table 7 largely reinforce the finding of no social returns in Table 6.[14]

5.3 Results using 1950 and 1990 data

Individual schooling is treated as exogenous in analyses using 1950 and 1990 data, since there is no quarter of birth information in these data sets. In principle, this may lead to biased estimates, although in practice, the estimates of social returns for 1960-80 are not sensitive to the exogeneity assumption. A second and potentially more serious problem is that the schooling variable in the 1990 Census is categorical, with no direct information on highest grade completed, so we have to use an imputed years of schooling measure for 1990. Since individual schooling has to be treated as exogenous when using 1990 data,

[14] A possible concern with the estimates in Tables 6 and 7 is that CSL dummies are correlated with measures of school quality. But since school quality is associated with higher wages, the omission of these variables could not be responsible for the apparent lack of a social return to education. In fact, controlling for the school quality variables used by Card and Krueger (1992a) leads to more negative estimates, though also less precise, than in our baseline specification. It should be noted, however, that even conditional on quality variables, there is a clear first-stage relationship between CSLs and average schooling.

the resulting measurement error may lead to biased estimates of social returns (see the discussion in Section 2.2).[15]

Table 8 reports estimates of social returns in the extended samples. Using child labor laws as instruments generates small positive or zero estimates of social returns with 1950-80 data. These estimates are more precise than those using 1960-80 data only. In column 1, for example, the estimated social return is .009 with a standard error of .025. As before, using compulsory attendance laws as instruments leads to somewhat larger estimates. But these estimates are less precise than those using CL instruments, and the first-stage relationships are not uniformly consistent with a causal interpretation of the correlation between CSLs and schooling. For example, in column 1, $CA9$ has a larger coefficient than both $CA10$ and $CA11$. Moreover, allowing private returns to schooling to vary by year and state leads to an estimated social return of only .017.

In contrast with the results using 1950-80 data, adding data from the 1990 census leads to statistically significant positive estimates of social returns when child labor laws are used as instruments. For example, the baseline estimate reported in column 2 shows a social return of .048 with a standard error of .02. Controlling for separate private returns by census year leads to an even larger social return of .074. Using CA instruments does not lead to significant estimates of social returns in the 1950-90 sample, though again the estimates using CA dummies are considerably less precise than the estimates using CL dummies.

The relatively large and precise social return estimates for 1950-90 generated by CL instruments may signal a change in the social value of human capital. But this result could also reflect changes in the schooling variable in 1990. The econometric discussion in Section 2.2 highlights the possibility of spurious social return estimates when the impact of individual schooling is poorly controlled. Measurement error in the 1990 schooling individual variable could generate a problem of this type.

To check whether measurement problems could be responsible for the 1950-90 results, we assigned mean values from the 1980 Census to a similar categorical schooling variable in the 1960, 1970 and 1980 Censuses, and then re-estimated social returns in 1960-80 treating individual schooling as exogenous.[16] This leads to larger estimates of social returns. For example, using CL instruments to estimate social returns with imputed

[15] A detailed description of the schooling variables used here appears in Appendix B.

[16] This exercise uses the IPUMS variable EDUCREC, which provides a uniform categorical schooling measure for the 1940-90 Censuses.

schooling data generates a social return of .024 instead of the estimate of .003 reported in Table 6. Similarly, using CA instruments generates a social return of .034 instead of .017 with the better measured schooling variable. These results suggest that the higher social returns estimated with 1990 data are likely due to changes in the way the education data were collected in 1990.

6 Changes in returns to schooling

In addition to affecting overall wage levels, changes in average schooling may affect the private returns to schooling. Mechanisms for this include externalities of the type discussed in Section 2.1. Suppose, for example, equation (2) is replaced by a general function $F(k_j, h_i)$. Assuming $F(k_j, h_i)$ is concave in k_j, all firms choose the same level of capital. This level is an increasing function of aggregate human capital, say, $k = G[H]$. Log wages can therefore be written as

$$\ln W_i \approx c + \ln F(G[H], h_i)$$

This more general functional form induces interaction terms between H and individual schooling, unless F is Cobb-Douglas as in Section 2.1. This can be viewed as generating a version of the additive social returns model, (7), where the coefficient on average schooling is a random function of h_i. Instrumental variables methods then estimate a weighted average of this random coefficient (see, for example, Angrist, Graddy, and Imbens, 1995).

A second and perhaps more important consideration is that even in frictionless competitive labor markets, the private returns to education change with average schooling when high and low education workers are imperfect substitutes (see, e.g., Freeman, 1976 and Katz and Murphy, 1992). In a neoclassical framework, part of what we are calling social returns includes a combination of declining returns to schooling and increased wages for less-educated workers in response to the increased supply of more-educated workers. In the appendix, we show that, as a theoretical matter, the net impact of these forces is to make the overall social return in an additive model larger. But since the impact of increasing education supplies is of independent interest, we briefly present empirical results from models with interaction terms between individual and average schooling.[17]

[17]For similar theoretical reasons, Moretti (1999) also estimates models with interactions between individual and average schooling.

When estimating models that include interactions terms between s_i and $\overline{S}_{jt}$, the $\overline{S}_{jt}$ main effects and interactions are treated as endogenous, and instrumented with CSL dummies and interactions between s_i and CSL dummies. For the purposes of this analysis, the s_i main effect was treated as exogenous. The interaction term is parameterized as $(s_i - 11) \cdot (\overline{S}_{jt} - 11)$, so the reported main effects are evaluated at 11 years of schooling. This is appropriate because CSL instruments have a large effect on high school graduation rates and because mean schooling is between 11-12 years. The models also include interactions between s_i and year and state main effects. This leads to a setup similar to Card and Krueger's (1992b) study of school quality, where the returns to schooling vary with cohort- and state-specific characteristics, conditional on additive cohort and state effects.

Panel A of Table 9 reports main effects and interaction terms estimated using 1960-80 data. The OLS estimates of social returns are about .05, and the interaction term suggests that private returns decline as average schooling levels rise. But both of these findings are reversed in the IV estimates. The social return main effects are negative and insignificant. Perhaps more surprisingly, CL instruments generate a positive and statistically significant interaction term of .029 (with other instruments, the interaction term is positive but not significant). This result is not consistent with a standard competitive model of the labor market, but is consistent with a model along the lines mentioned above (see also Acemoglu, 1999).

IV estimates of models with interaction terms using the 1950-80 sample, reported in Panel B, yield social return main effects that are positive, but imprecise and not statistically significant. The interaction term estimated with CL instruments is again statistically significant, and perhaps of a more plausible magnitude, suggesting that private returns to schooling increase by about .017 when average schooling increases by one year. Overall, however, the results with interaction terms are not very precise. We therefore leave a more detailed investigation of models with interactions between individual and average schooling for future work.

7 Concluding remarks

The notion that education has economic benefits is at the heart of education policy around the world. A large literature reports estimates of private returns to schooling on the order of 6-10 percent. However, private returns may be only part of the story. If there are

positive social returns to education, then private returns underestimate the economic value of schooling.

Identification of social returns requires exogenous variation in both individual and average schooling. In this paper, we use quarter of birth and state CSLs to generate this variation. These two sources are conceptually similar, since quarter of birth affects schooling because of CSLs as well. This identification strategy works because quarter of birth primarily affects individual schooling, while CSLs affect both individual and average schooling.

Estimates using CSLs as instruments for average schooling provide no evidence of substantial human capital externalities between 1960 and 1980; the estimated social returns range from -1 to less than 2 percent, and are not statistically different from 0. Adding data from 1950 leads to somewhat more precise estimates, without changing the basic pattern. Regressions using data from the 1990 Census, in contrast, generate statistically significant estimates of social returns of 4 percent or more with one set of instruments. This may reflect the increased importance of human capital after 1980. Further investigation, however, suggests that the larger estimates in samples with 1990 data are probably driven by increased measurement error due to changes in the schooling variable in the 1990 Census. On balance, therefore, there is little evidence for significant social returns to education over the range of variation induced by changing CSLs. This suggests that education policy for secondary schooling should be based largely on estimates of the private returns to education.

References

Acemoglu, Daron (1996), "A Microfoundation for Social Increasing Returns in Human Capital Accumulation," *Quarterly Journal of Economics* 111 [3], 779-804.

Acemoglu, Daron (1998), "Why Do New Technologies Complement Skills? Directed Technical Change and Wage Inequality," *Quarterly Journal of Economics* 113 [4], 1055-1089.

Acemoglu, Daron (1999), "Changes in Unemployment and Wage Inequality: An Alternative Theory and Some Evidence," forthcoming *American Economic Review.*

Angrist, Joshua, Kathryn Graddy, and Guido W. Imbens (1995), "Nonparametric Demand Analysis with an Application to the Demand for Fish," NBER Technical Working Paper No. 178, April, forthcoming *Review of Economic Studies.*

Angrist, Joshua D. and Guido W. Imbens (1995), "Two-Stage Least Squares Estimates of Average Causal Effects in Models with Variable Treatment Intensity," *Journal of the American Statistical Association* 90(430), 431-42.

Angrist, Joshua D. and Alan B. Krueger (1991), "Does Compulsory School Attendance Affect Schooling and Earnings?" *Quarterly Journal of Economics* 106, 979-1014.

Angrist, Joshua D. and Alan B. Krueger (1995), "Split-Sample Instrumental Variables Estimates of the Return to Schooling," *Journal of Business and Economic Statistics* 13, 225-235.

Angrist, Joshua D. and Alan B. Krueger (1999), "Empirical Strategies in Labor Economics," Chapter 23 in O. Ashenfelter and D. Card, eds., *The Handbook of Labor Economics Volume III*, Amsterdam: Elsevier.

Barro, Robert and Xavier Sala-i-Martin (1995), *Economic Growth*, McGraw Hill, New York.

Becker, Gary (1993), *Human Capital: A Theoretical and Empirical Analysis, with Special Reference to Education,* 3rd edition, Chicago: The University of Chicago Press.

Bils, Mark, and P.J. Klenow (1998), "Does Schooling Cause Growth or the Other Way Around?" NBER Working Paper No. 6393.

Borjas, George (1992), "Ethnic Capital and Intergenerational Mobility," *Quarterly Journal of Economics* 107, 123-150.

Bound, John, David Jaeger and Regina Baker (1995), "Problems with Instrumental Variables Estimation when the Correlation Between the Instruments and the Endogenous

Explanatory Variable is Weak," *Journal of the American Statistical Association* 90, 443-50.

Brock, William and Steve Durlauf (1999), "Interactions-Based Models" forthcoming in *The Handbook of Econometrics*, edited by James Heckman and Edward Leamer.

Card, David E. (1995a), "Earnings, Schooling and Ability Revisited," in Solomon W. Polachek, ed., *Research in Labor Economics.* Greenwich, CT: JAI Press.

Card, David E., (1995b), "Using Geographic Variation in College Proximity to Estimate the Return to Schooling," in L.N. Christofides, E.K. Grant, and R. Swidinsky, eds, *Aspects of Labor Market Behavior: Essays in Honour of John Vandekamp*, Toronto: University of Toronto press, 201-222.

Card, David E. (1999), "The Causal Effect of Education on Earnings," chapter in O. Ashenfelter and D. Card, eds, *The Handbook of Labor Economics* Volume III, Amsterdam: Elsevier.

Card, David E. and Alan Krueger (1992a), "School Quality and Black-White Relative Earnings: A Direct Assessment," *Quarterly Journal of Economics* 107, 151-200.

Card, David E. and Alan Krueger (1992b), "Does School Quality Matter? Returns to Education and the Characteristics of Public Schools in the United States," *Journal of Political Economy* 100, 1-40.

Chamberlain, Gary and Guido W. Imbens (1996) Hierarchical Bayes Models With Many Instrumental Variables," National Bureau of Economic Research Technical Paper 204, September.

Edwards, Linda (1978) "An Empirical Analysis of Compulsory Schooling Legislation," *Journal of Law and Economics*, 21, 203-22.

Ehrenberg, Ronald and Alan Marcus (1982), "Minimum Wages and Teenagers Enrollment-Employment Outcomes," *Journal of Human Resources* 27, 39-58.

Goldin, Claudia (1998), "America's Graduation from High School: The Evolution and Spread of Secondary Schooling in the Twentieth Century," *Journal of Economic History* 58, 345-74.

Hausman, Jerry (1978), "Specification Tests in Econometrics," *Econometrica* 46, 1251-1271.

Jacobs, Jane (1970), *The Economy of Cities*, New York, Vintage Books.

Katz, Lawrence F. and Kevin M. Murphy (1992), "Changes in Relative Wages, 1963-1987: Supply and Demand Factors," *Quarterly Journal of Economics*, 107(1):35-78

Krueger Alan and Mikael Lindahl (1998), "Education for Growth: Why and For Whom" Princeton Mimeo.

Lang, Kevin (1993) "Ability Bias, Discount Rate Bias and the Return to Education," Boston University Department of Economics, mimeo.).

Lang, Kevin, and David Kropp (1986), "Human Capital Versus Sorting: The Effects of Compulsory Attendance Laws," *Quarterly Journal of Economics* 101, 609-624.

Lucas, Robert (1988), "On the Mechanics of Economic Development," *Journal of Monetary Economics* 22, pp. 3-42.

Mankiw, N. Gregory, David Romer, and David N. Weil (1992), "A Contribution to the Empirics of Economic Growth," *Quarterly Journal of Economics* 107, 407-37.

Maré, David (1994), "Living with Graduates: Human Capital Interactions in Metropolitan Areas," New Zealand Department of Labour, mimeo.

Marshall, Alfred (1961), *Principles of Economics,* London, New York, MacMillan for the Royal Economic Society.

Moretti, Enrico (1999), "Estimating the External Return to Education: Evidence from Repeated Cross-sectional and Longitudinal Data," University of California, Berkeley Mimeo.

Margo, Robert A. (1990), *Race and Schooling in the South: 1880-1950,* Chicago: University of Chicago Press.

Margo, Robert A., and T. Aldrich Finegan (1996), "Compulsory Schooling Legislation and School Attendance in Turn-of-The Century America: A 'Natural Experiment' Approach," *Economics Letters* 53, 103-10.

Moulton, Brent R. (1986), "Random Group Effects and the Precision of Regression Estimates," *Journal of Econometrics* XXXII, 385-397.

Park, Jin Heum (1994), "Estimation of Sheepskin Effects and Returns to Schooling Using the Old and New CPS Measures of Educational Attainment," Princeton Industrial Relations Section Working Paper No. 338 (December).

Peri, Giovanni (1998), "Human Capital Externalities in US Cities," University of California, Berkeley, mimeo

Rauch, James E. (1993), "Productivity Gains from Geographic Concentration of Human Capital: Evidence from the Cities" *Journal of Urban Economics* 34, 380-400.

Ruggles, Steven, and Matthew Sobek (1997), *Integrated public use microdata series version 2.0, volume 1: user's guide* Historical Census projects, University of Minnesota,

Department of History.

Schmidt, Stefanie (1996), *School Quality, Compulsory Education Laws, and the Growth of American High School Attendance, 1915-1935*, MIT Ph.D. dissertation.

Staiger, Douglas and James H. Stock (1997), "Instrumental Variables Regression with Weak Instruments," *Econometrica* 65(3):557-86.

Topel, Robert (1999), "Labor Markets and Economic Growth" chapter in O. Ashenfelter and D. Card, eds., *The Handbook of Labor Economics* Volume III, Amsterdam: Elsevier.

Welch, Finis (1973), "Black-White Differences in Returns to Schooling," *American Economic Review* 63, 893-907.

Appendix A: Mathematical Details

A.1 Derivation of equation (10)

Rewrite equation (9) as follows

$$Y_{ij} = \mu^* + \pi_0 \tau_i + (\pi_0 + \pi_1)\overline{S}_j + \xi_i;$$

where $\tau_i \equiv s_i - \overline{S}_j$. Since τ_i and $\overline{S}_j$ are uncorrelated by construction, we have:

$$\begin{aligned} \rho_1 &= \pi_0 + \pi_1. \\ \pi_0 &= \frac{C(\tau_i, Y_{ij})}{V(\tau_i)}. \end{aligned}$$

Simplifying the second line,

$$\begin{aligned} \pi_0 &= \frac{C[(s_i - \overline{S}_j), Y_{ij}]}{[V(s_i) - V(\overline{S}_j)]} \\ &= \left[\frac{C(s_i, Y_{ij})}{V(s_i)}\right]\left[\frac{V(s_i)}{V(s_i) - V(\overline{S}_j)}\right] - \left[\frac{C(\overline{S}_j, Y_{ij})}{V(\overline{S}_j)}\right]\left[\frac{V(\overline{S}_j)}{V(s_i) - V(\overline{S}_j)}\right] \\ &= \rho_0\phi + \rho_1(1-\phi) = \rho_1 + \phi(\rho_0 - \rho_1) \end{aligned}$$

where $\phi \equiv \frac{V(s_i)}{V(s_i) - V(\overline{S}_j)}$. Solving for π_1, we have

$$\pi_1 = \rho_1 - \pi_0 = \phi(\rho_1 - \rho_0).$$

A.2. Relative Price Effects and Social Returns

A key ingredient in models with general equilibrium price effects is less-than-perfect substitution between labor types. Imperfect substitution is easiest to describe in a world with two schooling groups. Suppose labor markets are perfectly competitive, and workers have either high education (H) or low education (L). The aggregate production function is

$$Y = [A_l L^\rho + A_h H^\rho]^{\alpha/\rho} K^{1-\alpha}. \tag{14}$$

Normalize the price of capital to $1-\alpha$, so that $K = [A_l L^\rho + A_h H^\rho]^{1/\rho}$. Substituting into (14), gives $Y = [A_l L^\rho + A_h H^\rho]^{1/\rho}$. The wages of skilled and unskilled workers can therefore be written as

$$w_L = A_l\left[A_l + A_h(H/L)^\rho\right]^{(1-\rho)/\rho} \text{ and } w_H = A_h\left[A_l(H/L)^{-\rho} + A_h\right]^{(1-\rho)/\rho}$$

An increase in average schooling is equivalent to an increase in the share of H.

To see the implications of this set-up for our estimates of social returns, normalize the total population to 1, and let $H = S$, so that $L = 1 - S$. We interpret "social returns" in this model as the derivative of total log wage income with respect to S minus the effect of S due to private returns. This can be written as

$$\frac{d\ln W}{dS} - \ln\omega,$$

where W is the sum of log wages, $\ln W \equiv L\ln w_L + H\ln w_H = \ln w_L + S(\ln w_H - \ln w_L)$, and $\ln\omega = \ln w_H - \ln w_L$ is the private return to schooling. Differentiating, we have

$$\frac{d\ln W}{dS} = \ln\omega + S\frac{d\ln\omega}{dS} + \frac{d\ln w_L}{dS}. \tag{15}$$

This shows that the overall impact of changing S includes (i) the private return to schooling; (ii) a negative term due to declining returns to schooling since $\frac{d\ln\omega}{dS} < 0$; (iii) a positive term reflecting increased wages of less-educated workers.

The second and third terms in equation (15) can be simplified further to give

$$\frac{d\ln W}{dS} = \ln\omega + \frac{1}{(1-S)^2}\frac{1}{\sigma}\varsigma_l\left(\omega - 1\right) \tag{16}$$

where $\varsigma_l = \left[A_l L^\rho + A_h H^\rho\right]^{-1} A_l L^\rho$ is the share of low education workers in total labor costs and $\sigma \equiv \frac{1}{1-\rho}$ is the elasticity of substitution between skill groups. The expression $\frac{1}{(1-S)^2}\frac{1}{\sigma}\varsigma_l\left(\omega - 1\right)$ is always positive because $\omega = \frac{w_H}{w_L} > 1$. Therefore, increases in average education raise total wages by more than the private return to schooling unless $\sigma = \infty$, as in the model of Section 2.1. Intuitively, average wages go up by more than private returns because the wages of less educated workers increase more than enough to offset the decline in the wages of more educated workers.

Appendix B: Data sources and methods

1. Micro Data

The paper uses data from the 1950, 1960, 1970, 1980, and 1990 PUMS files. Census data were taken from the IPUMS system (Ruggles and Sobek, 1997). The files used are as follows:

1950 General (1/330 sample)
1960 General (1% sample)
1970 Form 1 State (1 % sample)
1970 Form 2 State (1% sample)
1980 5% State (A Sample)
1990 1% unweighted (a 1% random self-weighted sample created by IPUMS)

Our initial extract included all US-born white men aged 21-58. The 1950 sample is limited to "sample line" individuals (i.e., those with long-form responses). Our sample excludes men born or living in Alaska or Hawaii. Estimates were weighted by the IPUMS weighting variable SLWT, adjusted in the case of 1970 to reflect the fact that we use two files for that year (i.e., divided by 2). The weights are virtually constant within years, but vary slightly to reflect minor adjustments by IPUMS to improve estimation of population totals.

The schooling variable was calculated as follows: For 1950-80, the variable is HIGRADED (General), the IPUMS recode of highest grade enrolled and grade completed into highest grade completed. For the 1990 Census, which has only categorical schooling, we assigned group means for white men from Park (1994, Table 5), who uses a one-time overlap questionaire from theFeburary 1990 CPS to construct averages for essentially the same Census categories. This generates a years of schooling variable roughlt comparable across censuses (GRADCOMP). Finally, we censored GRADCOMP at 17 since this is the highest grade completed in the 1950 census. We call this variable GRADCAP.

The dependent variable is log weekly wage, calculated by dividing annual wages by weeks worked, where wages refer to wage and salary income only. Wage topcodes vary across censuses. We imposed a uniform topcode as follows. Wage data for every year for the full extract of white men aged 21-58 were censored at the 98th percentile for that year. The censoring value is the 98th percentile times 1.5. Weeks worked are grouped in the 1960 and 1970 censuses. We assigned means to 1960 categorical values using 1950 averages and we assigned means to 1970 categorical values using 1980 averages.

The analyses in the paper, including first-stage relationships, are limited to men with positive weekly wages. Analyses using 1960-80 data are limited to men born 1910-1919 in the 1960 Census, 1920-29 in the 1970 Census, and 1930-39 in the 1980 Census. Since year of birth variables are not available in the 1950 and 1990 censuses, analyses using those data sets are limited to men aged 40-49.

2. Calculation of average schooling

Average schooling is the mean of GRADCAP by state and census year for all US-born persons aged 16-64. For 1970, we used only the Form 2 State sample (a 1 % file) and for 1980 we used a 1% random subsample, drawn from the 5% State (A Sample) using the IPUMS SUBSAMP variable. The SLWT weighting variable was adjusted to reflect the fact that this leaves a 1% sample for each year. The averages use data excluding Alaska and Hawaii (residence or birthplace). Average schooling was calculated for individuals with positive weeks worked and weighted by the product of SLWT and weeks worked. Categorical weeks worked variables were imputed as described above.

3. Match to CSLs and state average schooling

The CSLs in force in each year from 1914-72 were measured using the five variables described in Section 4 of this appendix. For each individual in the microdata extract, we calculated the approximate year the person was age 14 using age on census day (not year of birth, which is not available in 1950 and 1990). The CSLs in force in that year in the person's state of birth were then assigned to that person. State average schooling was matched to individual state of residence and census year.

4. CSL variables

Data on CSLs were collected and organized by Ms. Xuanhui Ng, in consultation with us.

a. Table of sources

Year	*enroll_age*	*drop_age*	*req_sch*	*work_age*	*work_sch*
1914	Commissioner	Schmidt Commissioner	Schmidt	Schmidt	Schmidt
1917	Biennial	Biennial	Biennial	Biennial	Biennial
1921	Chart1-1921	Chart1-1921	Chart1-1921	Chart2-1921	Chart2-1921
1924	Chart1-1924	Chart1-1924	Chart1-1924	Chart2-1924	Chart2-1924
1929	M	#197	M	#197	#197
1935	Deffenbaugh;	Deffenbaugh; Schmidt	Deffenbaugh; Schmidt	Deffenbaugh; Schmidt	Deffenbaugh; Schmidt
1939	Umbeck	Umbeck	M	M	M
1946	SCLS-1946	SCLS-1946	SCLS-1946	SCLS-1946	SCLS-1946
1950	SCLS-1949 Keesecker-1950	SCLS-1949 Keesecker-1950	SCLS-1949 Keesecker-1950	SCLS-1949 Keesecker-1950	SCLS-1949 Keesecker-1950
1954	Keesecker-1955	Keesecker-1955	Keesecker-1955	M	Keesecker-1955
1959	SCLS-1960 Umbeck	SLCS-1960 Umbeck	SLCS-1960 Umbeck	SLCS-1960	SLCS-1960 Umbeck
1965	SLCS-1965 Steinhilber	SLCS-1965 Steinhilber	SLCS-1965 Steinhilber	SLCS-1965 LLS	SLCS-1965 Steinhilber

Notes: enroll_age is maximum age by which a child has to enroll at school.
drop_age is minimum age a child is allowed to drop out of school.
req_sch is minimum years of schooling a child has to obtain before dropping out.
work_age is the minimum age at which a child can get work permit.
work_sch is the minimum years of schooling a child needs for obtaining a work permit.

Source abbreviations are given with the references.

b. Methods

Data were drawn from the sources listed in the table of sources. In some cases sources were ambiguous or there were conflicts between sources for the same year. For resolution, we looked for patterns across years that seemed to make sense, and tried to minimize the number of source changes. In the source table, "M" denotes missing, i.e., we found no source or reliable information for this variable in this year. Missing data were imputed by bringing older data forward. Inter-source years were imputed and the data set expanded by bringing older data forward to make a complete set of 5 CSL laws for each year from 1914 to 1965.

The imputed data set contains either numerical entries or an "NR" indicating we found laws that appeared to impose no restriction (e.g., 6 years schooling required for a work permit, so *work_sch*=6, but a work permit available at any age, so *work_age*=NR). The algorithm for calculating required years of schooling for dropout and the required years of schooling for a work permit handles NR codes as follows:

If *req_sch*=NR, then *req_sch*=0;
If *enroll_age*=NR or *drop_age*=NR, then *ca*=max(0, *req_sch*);
If *enroll_age*≠NR and *drop_age*≠NR then *ca*=max(*drop_age-enroll_age*, *req_sch*).

If *work_age*=NR, then *work_age*=0;
If *work_sch*=NR, then *work_sch*=0;
If *enroll_age*=NR then *cl*=max(0, *work_sch*);
If *enroll_age*≠NR then *cl=max(work_age-enroll_age*, *work_sch*).

We coded a general literacy requirement without a specific grade or age requirement as NR. We coded a grade requirement of "elementary school" as 6, even though this was distinct from sixth grade in some sources (our dummies would group these requirements anyway).

5. References for Appendix B

[Deffenbaugh] Deffenbaugh, Walter S. and Keesecker, Ward, W., *Compulsory School Attendance Laws and Their Administration*, US Department of Interior, Office of Education, Bulletin 1935, No. 4, Washington: US GPO (1935).

[Keesecker-1950] Keesecker, Ward, W. and Allen, Alfred, C., *Compulsory School Attendance and Minimum Educational Requirements in the United States, 1950*, Federal Security Agency, Office of Education, Circular No. 278 (September 1950).

[Keesecker-1955] Keesecker, Ward, W. and Allen, Alfred, C., *Compulsory School Attendance and Minimum Educational Requirements in the United States*, US Department of Health, Education, and Welfare, Office of Education, Circular No. 440, Washington: US Department of Health, Education and Welfare (March 1955).

[Schmidt] Schmidt, Stefanie, R., *School Quality, Compulsory Education Laws, and the Growth of American High School Attendance*, 1915-35, MIT Ph.D. dissertation (1996).

[Steinhilber] Steinhilber, A.W. and Sokolowski, C.J., *State Law on Compulsory Attendance,* US Department of Health, Education and Welfare, Office of Education, Circular 793, Washington: US GPO (1966).

[Umbeck] Umbeck, Nelda, *State Legislation on School Attendance and Related Matters - School Census and Child Labor*, US Department of Health, Education and Welfare, Office of Education, Circular No. 615, Washington: US GPO (January 1960).

[Biennial] US Department of the Interior, Bureau of Education, *Biennial Survey of Education 1916-18.* Bulletin 1919, No. 90 (1921).

[Commissioner] US Department of the Interior, Office of Education, *Report of the Commissioner of Education for the Year Ended June 30, 1917*, Vol. 2, p. 69, Washington: US GPO.

[LLS] U.S. Department of Labor, Bureau of Labor Standards, *Summary of State Child Labor Laws,* Labor Law Series No. 3-A, Washington: US Department of Labor (September 1966).

[SCLS-1946] U.S. Department of Labor, Division of Labor Standards, *State Child-Labor Standards: A State-by-State Summary of Laws Affecting the Employment of Minors Under 18 Years Of Age,* Child Labor Series No. 2, Washington: US GPO (July 1946).

[SCLS-1949] _____, *State Child-Labor Standards: A State-by-State Summary of Laws Affecting the Employment of Minors Under 18 Years of Age*, Bulletin 114, Washington: US GPO (September 1949).

[SCLS-1960] _____, *State Child-Labor Standards: A State-by-State Summary of Laws Affecting the Employment of Minors Under 18 Years of Age*, Bulletin 158 (Revised 1960), Washington: US GPO (May 1960).

[SCLS-1965] _____, *State Child-Labor Standards: A State-by-State Summary of Laws Affecting the Employment of Minors Under 18 Years of Age*, Bulletin 158 (Revised 1965), Washington: US GPO (September 1965).

[Chart1-1921] US Department of Labor, Children's Bureau, *State Child-Labor Standards, January 1, 1921*," Chart Series No. 1, Washington: US GPO.

[Chart1-1924] _____, *State Child-Labor Standards*, January 1, 1924, Chart Series No. 1, Washington: US GPO.

[Chart2-1921] _____, *State Compulsory School Attendance Standards Affecting the Employment of Minors*, January 1, 1921, Chart Series No. 2, Washington: US GPO.

[Chart2-1924] _____, "State Compulsory School Attendance Standards affecting the employment of minors, January 1, 1924," Chart Series No. 2, Washington: US GPO.

[#197] _____ *Child Labor Facts and Figures (October 1929)* Bulletin 197, Washington: US GPO.

Table 1
Descriptive Statistics for Census IPUMS

Variables	1950	1960	1970	1980	1990
		QOB Samples			
		Covariates			
Age	44.16 (2.87)	44.55 (2.88)	44.74 (2.90)	44.66 (2.94)	44.10 (2.84)
Individual Education	9.67 (3.40)	10.52 (3.22)	11.59 (3.18)	12.62 (2.98)	13.70 (2.49)
		Regressors			
State Average Education	9.94 (0.72)	10.65 (0.54)	11.52 (0.41)	12.46 (0.30)	13.10 (0.23)
Lowest State Average Education	7.87 [MS]	9.24 [MS]	10.45 [SC]	11.81 [KY]	12.62 [AR]
Highest State Average Education	11.18 [UT]	11.80 [UT]	12.38 [UT]	13.07 [DC]	13.74 [DC]
		Dependent Variable			
Log Weekly Wage	4.06 (0.77)	4.64 (0.63)	5.17 (0.65)	5.90 (0.72)	6.44 (0.73)
		Instruments			
Percent Child Labor 6	0.45	0.23	0.19	0.05	0.03
Percent Child Labor 7	0.45	0.36	0.24	0.24	0.16
Percent Child Labor 8	0.10	0.36	0.50	0.41	0.37
Percent Child Labor 9+	0.01	0.05	0.07	0.31	0.44
Percent Compulsory Attendance 8	0.57	0.35	0.24	0.11	0.11
Percent Compulsory Attendance 9	0.40	0.53	0.44	0.44	0.44
Percent Compulsory Attendance 10	0.02	0.06	0.08	0.09	0.06
Percent Compulsory Attendance 11+	0.01	0.07	0.24	0.37	0.39
N	16659	72344	161029	376479	103184

Notes: Standard deviations are in parentheses. Bracketed entries in the 'Lowest State Average Education' and 'Highest State Average Education' rows are abbreviations indicating the state with the lowest and highest average schooling. All other entries are means. The data are from the Census IPUMS for 1960 through 1980, with the sample restricted to white males aged 40-49 in the Census year.

Table 2
OLS Estimates of Private and Social Returns to Schooling

	1960-1980	1950-1980	1950-1990	1950	1960	1970	1980	1990
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
				A. Private Returns				
Private Return to Schooling	0.073 (0.0003)	0.068 (0.0003)	0.075 (0.0003)	0.055 (0.002)	0.069 (0.001)	0.076 (0.001)	0.075 (0.001)	0.102 (0.001)
State of Residence Main Effects?	YES	YES	YES	NO	NO	NO	NO	NO
				B. Private and Social Returns				
Private Return to Schooling	0.073 (0.000)	0.068 (0.000)	0.074 (0.000)	0.055 (0.002)	0.068 (0.001)	0.075 (0.001)	0.074 (0.000)	0.102 (0.001)
Social Return to Schooling	0.073 (0.016)	0.061 (0.004)	0.072 (0.003)	0.136 (0.017)	0.136 (0.016)	0.128 (0.021)	0.160 (0.027)	0.168 (0.047)
State of Residence Main Effects?	YES	YES	YES	NO	NO	NO	NO	NO
N	609852	626511	729695	16659	72344	161029	376479	103184

Notes: Standard errors corrected for state-year clustering are shown in parentheses. The data are from the Census IPUMS for 1950 through 1990, with the sample restricted to white males aged 40-49 in the Census year. All regressions contain Census year, year of birth, and state of birth main effects.

Table 3
Description of Child Labor and Compulsory Schooling Laws

Year at Age 14 (Census Year)	Earliest Drop Out Age	Latest Enrollment Age	Minimum Schooling for Dropout	Earliest Work Age	Required Schooling for Work Permit
	(1)	(2)	(3)	(4)	(5)
1914 (50)	15.31	7.49	1.90	11.00	1.70
	(1.20)	(0.52)	(3.40)	(5.75)	(2.56)
1917 (50)	15.55	7.63	1.93	13.43	2.98
	(0.89)	(0.49)	(2.74)	(1.98)	(2.66)
1921 (50)	15.69	7.42	4.28	13.94	4.19
	(0.99)	(0.51)	(3.63)	(1.71)	(2.97)
1924 (60)	15.88	7.29	5.64	14.11	4.91
	(0.97)	(0.57)	(3.64)	(1.33)	(3.04)
1929 (60)	15.97	7.30	5.66	14.16	5.31
	(0.93)	(0.58)	(3.62)	(1.33)	(3.01)
1935 (70)	15.96	7.24	7.24	14.14	6.02
	(0.94)	(0.55)	(3.73)	(0.76)	(2.67)
1939 (70)	16.16	7.16	7.29	14.15	6.01
	(1.05)	(0.51)	(3.74)	(0.77)	(2.70)
1946 (80)	16.31	7.09	7.91	14.77	4.67
	(0.63)	(0.53)	(4.00)	(1.16)	(3.37)
1950 (80)	16.27	7.08	7.94	15.03	3.51
	(0.60)	(0.53)	(4.49)	(1.14)	(3.47)
1954 (80)	16.30	7.05	7.79	15.02	4.06
	(0.63)	(0.52)	(4.65)	(1.20)	(3.67)
1959 (90)	16.25	7.05	7.40	15.19	3.49
	(0.60)	(0.53)	(4.79)	(1.19)	(3.56)
1964 (90)	16.20	7.05	7.44	15.17	3.51
	(0.60)	(0.54)	(4.79)	(1.22)	(3.57)

Notes: Standard deviations are in parentheses. All other entries are means. The data are from the Census IPUMS for 1950 through 1990, with the sample restricted to white men aged 40-49 in the Census year. See the data appendix for sources and method.

Table 4a
The Effect of Compulsory Attendance Laws and Child Labor Laws on Individual Schooling

	Including State of Residence Controls				Without State of Residence Controls			
	1960-1980	1950-1980	1950-1990	1980	1960-1980	1950-1980	1950-1990	1980
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
				Child Labor Laws				
CL 7	0.095	0.117	0.173	0.050	0.105	0.115	0.175	0.062
	(0.030)	(0.024)	(0.021)	(0.041)	(0.077)	(0.051)	(0.043)	(0.041)
CL 8	0.124	0.130	0.213	0.132	0.120	0.119	0.202	0.143
	(0.034)	(0.032)	(0.026)	(0.034)	(0.093)	(0.075)	(0.059)	(0.034)
CL 9	0.259	0.220	0.398	0.167	0.269	0.225	0.410	0.182
	(0.039)	(0.038)	(0.028)	(0.041)	(0.098)	(0.084)	(0.059)	(0.041)
				Compulsory Attendance Laws				
CA 8	0.117	0.083	0.189	-0.011	0.103	0.068	0.171	-0.009
	(0.027)	(0.025)	(0.020)	(0.034)	(0.072)	(0.057)	(0.043)	(0.034)
CA 9	0.095	0.059	0.113	0.100	0.106	0.074	0.133	0.104
	(0.034)	(0.036)	(0.020)	(0.044)	(0.085)	(0.077)	(0.063)	(0.045)
CA 10	0.167	0.144	0.260	0.115	0.184	0.165	0.290	0.119
	(0.038)	(0.036)	(0.028)	(0.037)	(0.103)	(0.085)	(0.063)	(0.038)
N	609852	626511	729695	376479	609852	626511	729695	376479

Notes: Standard errors corrected for state-year clustering are shown in parentheses. The data are from the Census IPUMS for 1950 through 1990, with the sample restricted to white males aged 40-49 in the Census year. All regressions contain Census year, year of birth, and state of birth main effects.

Table 4b
Effects of Child Labor and Compulsory Schooling Laws on Discrete Levels of Schooling

	Results for 1960-1980					Results for 1950-1980				
	Completed 8 Years or Higher	Completed 10 Years or Higher	Completed 12 Years or Higher	Completed 14 Years or Higher	Completed 16 Years or Higher	Completed 8 Years or Higher	Completed 10 Years or Higher	Completed 12 Years or Higher	Completed 14 Years or Higher	Completed 16 Years or Higher
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Dependent Variable Mean	0.908	0.747	0.617	0.249	0.167	0.884	0.695	0.562	0.226	0.151
Child Labor Laws										
CL 7	0.019	0.019	0.014	-0.005	-0.005	0.031	0.014	0.009	-0.004	-0.004
	(0.004)	(0.005)	(0.005)	(0.006)	(0.004)	(0.004)	(0.004)	(0.003)	(0.004)	(0.003)
CL 8	0.032	0.023	0.018	-0.014	-0.014	0.033	0.019	0.016	-0.009	-0.010
	(0.005)	(0.005)	(0.005)	(0.007)	(0.046)	(0.005)	(0.005)	(0.005)	(0.006)	(0.003)
CL 9	0.061	0.045	0.035	-0.019	-0.018	0.065	0.034	0.024	-0.021	-0.007
	(0.005)	(0.006)	(0.006)	(0.007)	(0.052)	(0.005)	(0.006)	(0.006)	(0.007)	(0.004)
Compulsory Attendance Laws										
CA 8	0.036	0.014	0.010	-0.009	-0.011	0.032	0.010	0.006	-0.010	-0.010
	(0.004)	(0.004)	(0.004)	(0.005)	(0.004)	(0.004)	(0.004)	(0.004)	(0.005)	(0.003)
CA 9	0.020	0.023	0.025	-0.011	-0.008	0.016	0.022	0.022	-0.011	-0.009
	(0.004)	(0.005)	(0.005)	(0.006)	(0.005)	(0.005)	(0.005)	(0.005)	(0.006)	(0.005)
CA 10	0.030	0.034	0.037	-0.013	-0.009	0.022	0.032	0.032	-0.010	-0.005
	(0.005)	(0.006)	(0.006)	(0.007)	(0.005)	(0.005)	(0.006)	(0.005)	(0.007)	(0.005)

Notes: Standard errors corrected for state-year clustering are shown in parentheses. All entries are OLS estimates from a regression of a dummy for having completed the indicated year of schooling on child labor law or compulsory attendance law dummies. All regressions also contain Census year, year of birth, state of birth, and state of residence main effects. The data are from the Census IPUMS for 1950 through 1980, with the sample restricted to white males aged 40-49 in the Census year. The sample size for the 1960-1980 columns is 609,852; the sample size for the 1950-1980 columns is 626,511.

Table 5
2SLS Estimates of Private Returns to Schooling

	QOB Instruments			CSL Instruments					
				CL Instruments			CA Instruments		
	1960-1980	1980	1960-1970	1960-1980	1950-1980	1950-1990	1960-1980	1950-1980	1950-1990
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Including State of Residence Main Effects	0.073 (0.012)	0.090 (0.016)	0.063 (0.017)	0.076 (0.034)	0.103 (0.038)	0.113 (0.018)	0.092 (0.044)	0.099 (0.052)	0.081 (0.023)
No State of Residence Main Effects	0.073 (0.012)	0.088 (0.016)	0.063 (0.017)	0.080 (0.064)	0.112 (0.060)	0.126 (0.027)	0.101 (0.088)	0.094 (0.086)	0.100 (0.040)
N	609852	376479	233373	609852	626511	729695	609852	626511	729695

Notes: Standard errors corrected for state-year clustering are in parentheses. All entries are two-stage least squares estimates of private returns to schooling, using the excluded instruments indicated above and discussed in the text. The data are from the Census 1PUMS for 1950 through 1990, with the sample restricted to white males aged 40-49 in the Census year. 'QOB' refers to the set 30 dummies interacting quarter of birth and year of birth. . 'CL' refers to a set of dummies indicating state and year specific child labor laws. 'CA' refers to a set of dummies indicating state and year specific compulsory attendance laws. All models contain Census year, year of birth, and state of birth main effects.

Table 6

2SLS Estimates of Private and Social Returns to Schooling

	Individual Schooling Endogenous				Individual Schooling Exogenous			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Second-Stage Estimates							
Instrument Set	QOB & CL	QOB & CA	QOB, CA & CL	QOB, CA & CL, Interactions	CL	CA	CL & CA	QOB, CA & CL, interactions
Private Return to Schooling	0.074 (0.012)	0.074 (0.012)	0.075 (0.012)	0.060 (0.013)	0.073 (0.0003)	0.073 (0.0003)	0.073 (0.0003)	0.073 (0.0003)
Social Return to Schooling	0.003 (0.040)	0.017 (0.043)	0.004 (0.035)	0.005 (0.033)	0.002 (0.038)	0.018 (0.042)	0.006 (0.033)	-0.011 (0.030)
	First-Stage for State-Year Average Schooling							
CL 7	0.080 (0.028)		0.062 (0.025)		0.084 (0.028)		0.062 (0.025)	
CL 8	0.107 (0.035)		0.068 (0.031)		0.107 (0.035)		0.068 (0.031)	
CL 9	0.227 (0.036)		0.184 (0.034)		0.226 (0.035)		0.183 (0.034)	
CA 9		0.128 (0.026)	0.102 (0.023)			0.128 (0.026)	0.104 (0.030)	
CA 10		0.122 (0.030)	0.104 (0.029)			0.122 (0.030)	0.104 (0.029)	
CA 11		0.144 (0.038)	0.094 (0.036)			0.143 (0.038)	0.094 (0.036)	

Notes: Standard errors corrected for state-year clustering are reported in parentheses. All entries are two-stage least squares estimates of returns to schooling, using the excluded instruments indicated above and discussed in the text. 'QOB' refers to a set of dummies interacting quarter of birth and year of birth. 'CL' refers to a set of dummies indicating state and year specific child labor laws. 'CA' refers to a set of dummies indicating state and year specific compulsory attendance laws. The data are from the Census IPUMS for 1960 through 1980, with the sample restricted to white males aged 40-49 in the Census year. All regressions contain Census year, year of birth, state of birth, and state of residence main effects. The sample size for all columns is 609852.

Table 7

2SLS Estimates of Social Returns to Schooling: Additional Results

	Individual Education Endogenous			1960-1980 Individual Education Exogenous			
	Baseline 1960-1980	1960 & 1980 Only	1970 & 1980 Only	Private Returns Separate by Census	Private Returns Separate by Census and State	Private Returns=.08	Private Returns=.09
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
			A. Results Using Child Labor Laws as Instruments				
Social Return to Schooling	0.003 (0.040)	-0.006 (0.036)	0.013 (0.052)	0.007 (0.039)	-0.024 (0.039)	-0.006 (0.038)	-0.018 (0.039)
			First Stage for State-Year Average Schooling				
CL 7	0.080 (0.028)	0.140 (0.041)	0.057 (0.032)	0.083 (0.028)	0.080 (0.026)	0.084 (0.028)	0.084 (0.029)
CL 8	0.107 (0.035)	0.164 (0.044)	0.074 (0.034)	0.104 (0.034)	0.100 (0.031)	0.107 (0.035)	0.107 (0.035)
CL 9	0.227 (0.036)	0.311 (0.046)	0.154 (0.033)	0.223 (0.035)	0.210 (0.032)	0.227 (0.036)	0.227 (0.035)
			B. Results Using Compulsory Attendance Laws as Instruments				
Social Return to Schooling	0.017 (0.043)	0.055 (0.035)	0.047 (0.049)	0.021 (0.043)	-0.018 (0.043)	0.011 (0.042)	0.010 (0.043)
			First-Stage for State-Year Average Schooling				
CA 9	0.128 (0.026)	0.223 (0.040)	0.090 (0.018)	0.125 (0.026)	0.118 (0.023)	0.128 (0.026)	0.128 (0.026)
CA10	0.122 (0.030)	0.260 (0.048)	0.108 (0.023)	0.120 (0.030)	0.112 (0.027)	0.122 (0.030)	0.122 (0.030)
CA 11	0.144 (0.038)	0.239 (0.051)	0.142 (0.031)	0.141 (0.037)	0.134 (0.034)	0.143 (0.038)	0.143 (0.038)
			C. OLS Estimates				
Social Return to Schooling	0.073 (.0003)	0.081 (0.015)	0.011 (0.027)	0.079 (0.017)	0.044 (0.016)	0.069 (0.017)	0.063 (0.017)
N	609852	448823	537508	609852	969877	609852	609852

Notes: Standard errors corrected for state-year clustering are reported in parentheses. All entries are estimates of returns to schooling, using dummies for child labor laws or compulsory attendance laws as excluded instruments. The data are from the Census IPUMS. Unless otherwise indicated, the sample is restricted to white males aged 40-49 in the Census year. All regressions contain individual education, Census year, year of birth, state of birth, and state of residence main effects.

Table 8
2SLS Estimates of Private and Social Returns to Schooling : Additional Samples, Individual Education Treated As Exogenous

	Baseline Results		Separate Private Returns by Census		Separate Private Returns by Census and State	
	50-80 (1)	50-90 (2)	50-80 (3)	50-90 (4)	50-80 (5)	50-90 (6)
	A. Results Using Child Labor Laws as Instruments					
Social Return	0.009 (0.025)	0.048 (0.019)	0.023 (0.025)	0.074 (0.019)	-0.034 (0.025)	0.041 (0.021)
	First-Stage for State-Year Average Schooling					
CL 7	0.173 (0.024)	0.165 (0.019)	0.170 (0.023)	0.162 (0.019)	0.158 (0.020)	0.145 (0.016)
CL 8	0.126 (0.036)	0.144 (0.027)	0.123 (0.035)	0.139 (0.027)	0.113 (0.031)	0.121 (0.022)
CL 9	0.278 (0.039)	0.333 (0.026)	0.275 (0.039)	0.327 (0.026)	0.250 (0.034)	0.280 (0.022)
	B. Results Using Compulsory Attendance Laws as Instruments					
Social Return	0.040 (0.038)	0.0006 (0.027)	0.053 (0.039)	0.038 (0.027)	0.017 (0.038)	-0.008 (0.029)
	First- Stage for State-Year Average Schooling					
CA 9	0.133 (0.028)	0.172 (0.019)	0.130 (0.027)	0.168 (0.019)	0.118 (0.023)	0.143 (0.015)
CA 10	0.106 (0.037)	0.167 (0.028)	0.105 (0.036)	0.164 (0.027)	0.096 (0.031)	0.139 (0.022)
CA 11	0.096 (0.042)	0.182 (0.029)	0.095 (0.041)	0.178 (0.028)	0.087 (0.036)	0.154 (0.023)
	C. OLS Estimates					
Social Return	0.061 (0.009)	0.072 (0.006)	0.076 (0.009)	0.094 (0.007)	0.039 (0.008)	0.057 (0.004)
N	626510	729695	626510	729695	626510	729695

Notes: Standard errors corrected for state-year clustering are reported in parentheses. Estimates of social returns to schooling use dummies for child labor and compulsory attendance laws as excluded instruments. Individual schooling is treated as exogenous. The sample is restricted to white males aged 40-49 in the Census year. All regressions contain individual schooling, Census year, year of birth, state of birth, and state of residence main effects.

Table 9
Estimates of Social Return to Schooling and Changes in Private Returned to Schooling

	OLS	CL	CA	CA & CL
	(1)	(2)	(3)	(4)
		A. 1960-1980		
Social Return to Schooling	0.053 (0.016)	-0.029 (0.038)	-0.015 (0.043)	-0.012 (0.032)
Interaction term	-0.021 (0.002)	0.029 (0.009)	0.015 (0.009)	0.011 (0.007)
		B. 1950-1980		
Social Return to Schooling	0.034 (0.009)	0.020 (0.025)	0.065 (0.038)	0.031 (0.022)
Interaction term	-0.015 (0.001)	0.017 (0.006)	0.006 (0.008)	0.005 (0.005)

Notes: Standard errors corrected for state-year clustering are in parentheses. In Panel A, the interaction term is parameterized as (Individual Education-11)*(State-Year Average Education-11), and in Panel B, it is parameterized as High School Graduate Dummy*(State-Year Average Education-11), so the estimates of individual and average schooling main effects are evaluated at 11 years of individual and average schooling. Excluded instruments in Panel A are individual schooling interacted with CL and CA dummies. Excluded instruments in Panel B are a high school graduate dummy interacted with CL and CA dummies. All regressions contain Census year, year of birth, state of birth, and state of residence main effects.

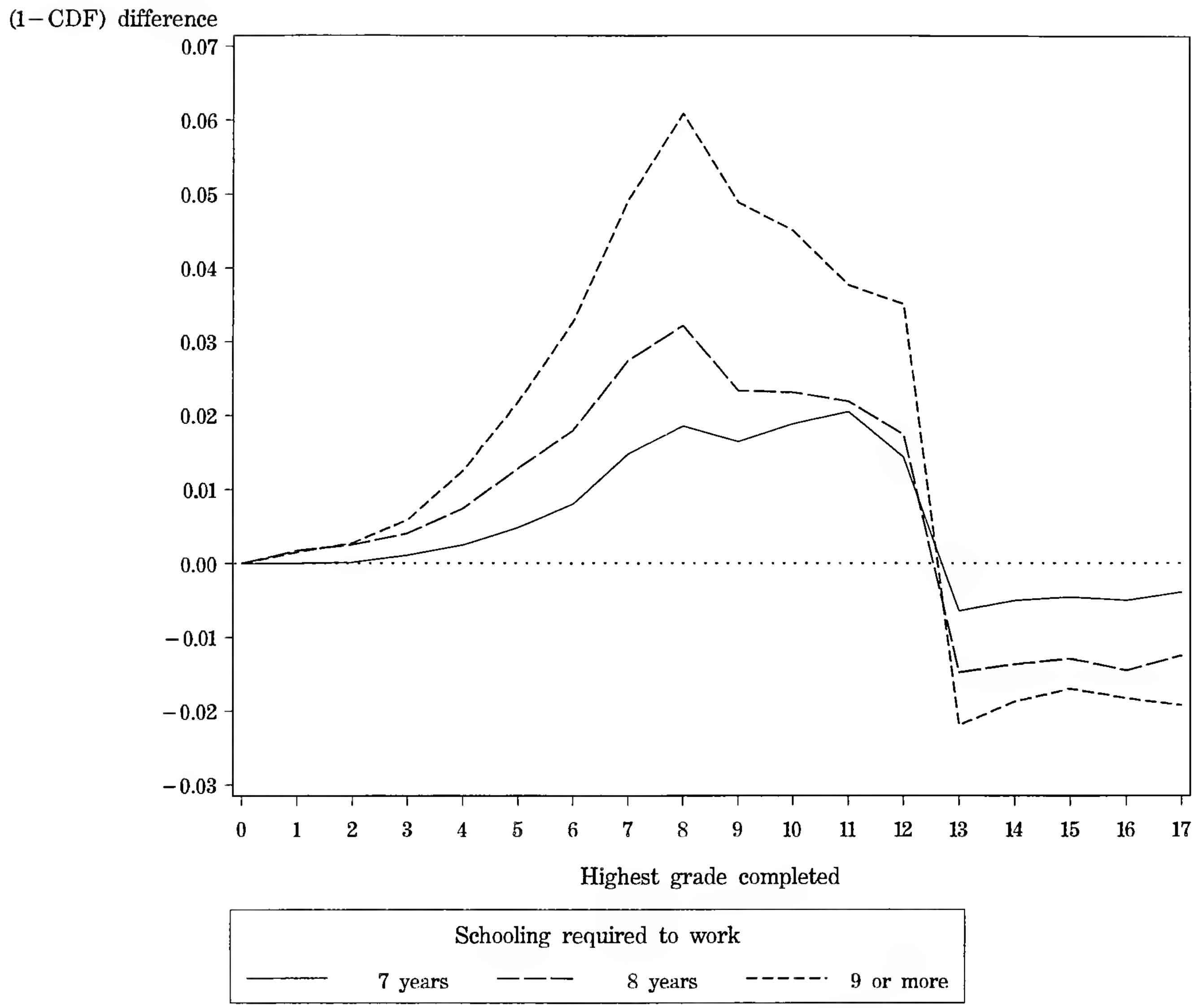


Figure 1. CDF difference by severity of child labor laws. The figure shows the difference in the probability of schooling at or exceeding the grade level on the X−axis. The reference group is 6 or fewer years required schooling.

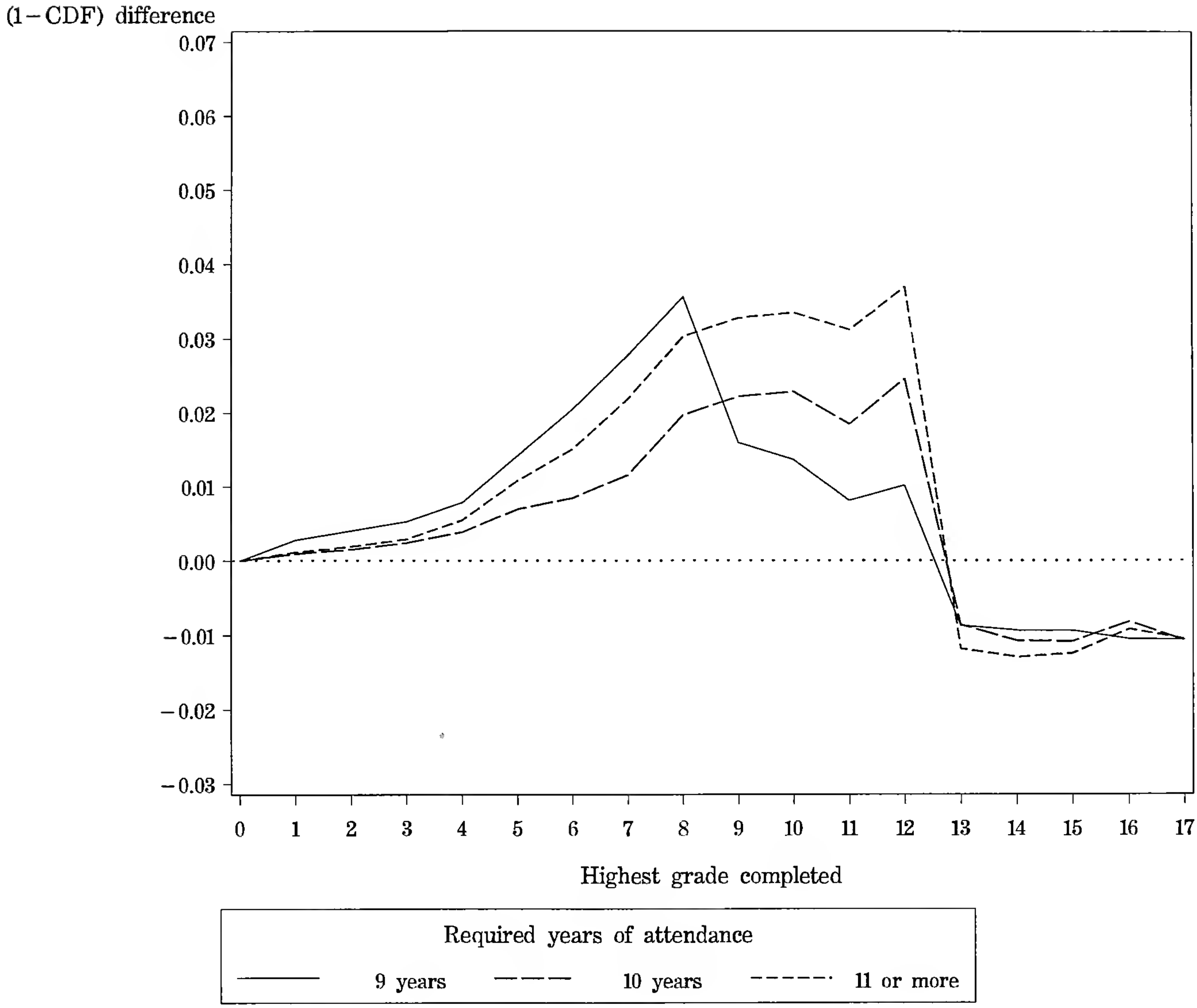
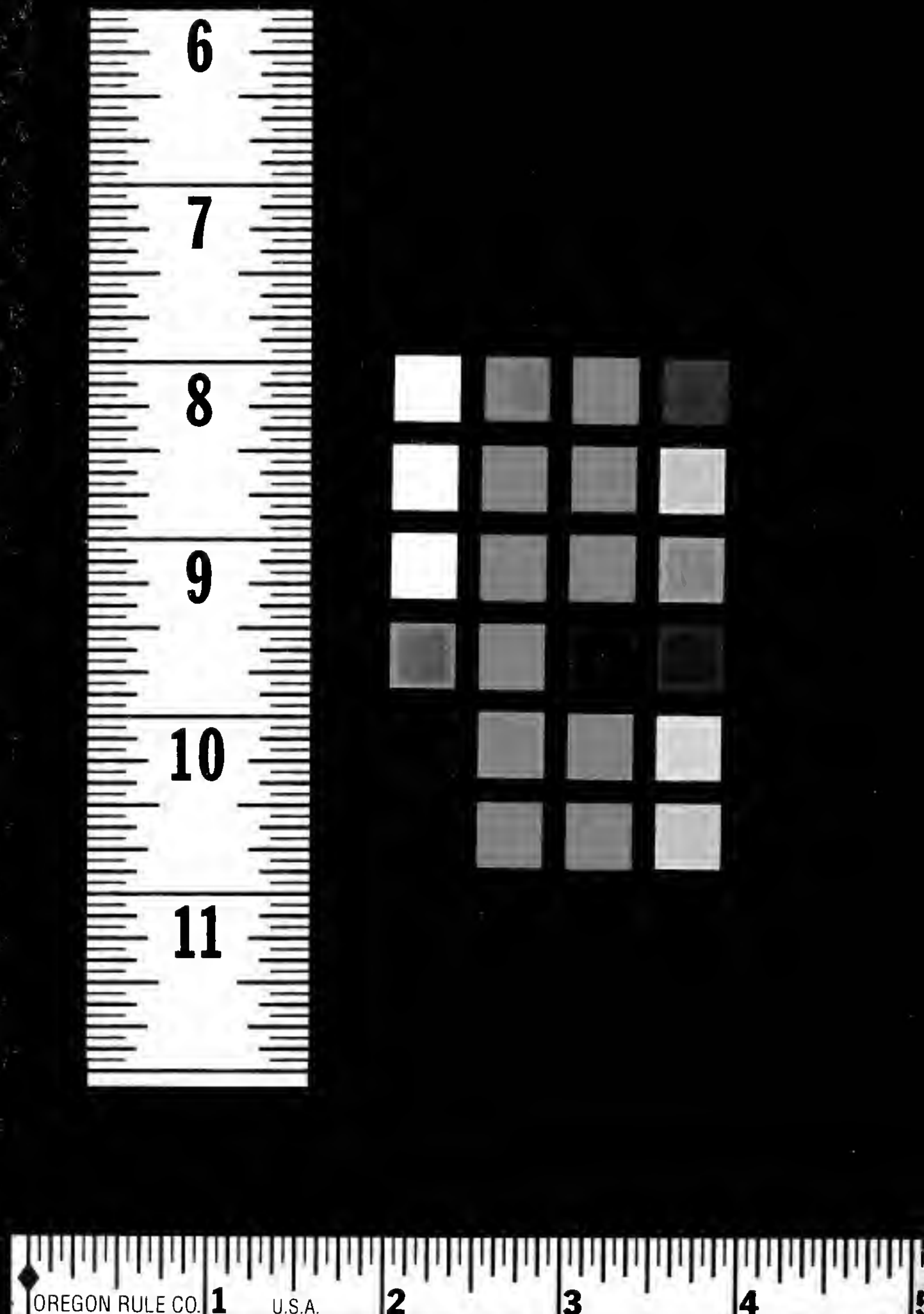


Figure 2. CDF difference by severity of compulsory attendance laws. The figure shows the difference in the probability of schooling at or exceeding the grade level on the X−axis. The reference group is 8 or fewer years required schooling.

DEC

Date Due

Lib-26-67


6
7
8
9
10
11
OREGON RULE CO.
1
U.S.A.
2
3
4